Credit Crisis and Strategic Human Capital Decisions: Evidence from Micro-Finance Regulation in India

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Abstract

This paper provides causal evidence on how households reallocate human capital investments in response to financial distress. We analyze the 2010 Andhra Pradesh microfinance ban, which abruptly halted microcredit operations, constraining loan recovery and liquidity. Using a synthetic difference-in-differences strategy, we estimate that the ban led to a six-percentage-point decline in school enrollment, with heterogeneous effects across grade levels. While primary, middle, and secondary school enrollment fell, higher secondary school enrollment increased, suggesting that households prioritize older children's education when liquidity constraints tighten. This reallocation extends to child labor decisions—younger children were more likely to enter wage work, while older children withdrew from the labor force. The results highlight dynamic complementarities in education, where prior schooling investments shape future human capital decisions, reinforcing the need for targeted policies to protect younger children's education in credit-constrained settings.

Keywords: Human Capital, Dynamic Complementarity in Education, Child Labor, Micro-Credit

JEL codes: G21, J13, O15, J21

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1 Introduction

The relationship between economic shocks and human capital investment has been widely studied,[1] yet how households strategically allocate resources among their children in response to financial distress remains underexplored. Policymakers and researchers are particularly concerned about how fluctuations in income affect schooling decisions and child labor supply, but much of the literature assumes uniform effects across all children within a household. While existing research documents both positive and negative effects of income changes on human capital investment (e.g., Baland and Robinson (2000); Dehejia and Gatti (2002)), it largely overlooks the role of intra-household strategic behavior—specifically, how parents adjust educational investments across children when financial constraints bind.

This paper challenges the assumption that households distribute human capital investments equally among siblings, an assumption prevalent in many models of child labor and education. In multi-child households, which are particularly common in rural developing economies, parents must make differentiated investment decisions when faced with financial shocks. Data from the India Human Development Survey shows that more than half of households have multiple children under 18, making these strategic decisions highly relevant. During periods of economic hardship, parents face a trade-off: older children may be prioritized for continued schooling due to higher expected returns on education, but they may also be pulled into the labor force due to their greater earning potential. These competing incentives create theoretical ambiguity—does financial distress lead households to protect prior investments in older children's education, or do they rely more on older children for income? Understanding this intra-household reallocation is critical for designing effective education and labor policies in credit-constrained economies.

In order to empirically assess this trade-off, we leverage a ban on microfinance activities of private organizations in the state of Andhra Pradesh (AP), India, which contracted the available rural credit by approximately 1 billion USD. Using the spatial and temporal variation in the credit supply generated by this shock, we analyze its impact on children's human capital attainment and labor supply.[2]

We collected data from various sources to identify districts with microfinance lenders

[1] Jacoby and Skoufias (1997); Jensen (2000); Thomas, Beegle, Frankenberg, Sikoki, Strauss and Teruel (2004); Maccini and Yang (2009); Hossain (2023); Shah and Steinberg (2021); Li and Sekhri (2020); Edmonds and Theoharides (2020); Shah and Steinberg (2021).

[2] Breza and Kinnan (2021) study the impact of this shock and show that this leads to a substantial decline in wages.

prior to the 2010 ban, during a period of rapid industry expansion. In 2008, of the 640 districts in India, 208 had private microcredit lending institutions (MFIs) (see Figure 1a). Our empirical analysis focuses on these 208 districts, including those in AP, the state directly affected by the ban. While the ban completely halted microfinance operations in AP, other districts were indirectly affected to varying degrees. Breza and Kinnan (2021) show that the AP regulatory shock propagated to other regions. However, in a subset of districts, the shock did not spread at all. We use these unaffected districts that had at least one MFI in 2008 as our empirical counterfactual.

For our empirical analysis, we link microfinance exposure data with three additional sources. We collect school enrollment data from the *Annual Status of Education Report* (ASER) survey and labor force participation data from the *National Sample Survey Organization* (NSSO). Finally, to examine household-level mechanisms, we use household panel data from two rounds of the *India Human Development Survey* (IHDS).

Our identification strategy compares MFI districts in Andhra Pradesh (treated districts) with MFI districts in other states that were entirely unaffected by the ban (control districts) before and after the 2010 microfinance ban. We employ the synthetic difference-in-differences approach proposed by Arkhangelsky et al. (2021) to construct a synthetic control from the unaffected districts where the MFI industry was active. This approach combines the benefits of both the synthetic control methods (Abadie and Gardeazabal, 2003) and the difference-in-difference strategy, and, by construction, guaranteeing parallel pre-trends. Our event study analysis corroborates the lack of differential pre-trends.

Our empirical findings indicate that the microfinance ban led to a significant decline in children's school enrollment, resulting in 1.1 million children dropping out.[3] Additionally, we observe that the investment decisions of parents differ based on the ages and grade levels of children, with younger children experiencing a higher likelihood of dropping out. In the case of one-child households, the younger the child, the higher the probability of the child dropping out of school. By sharp contrast, older children in higher secondary schools are more likely to remain enrolled. To further examine the mechanism, we use the IHDS household panel data. Our analysis shows that in multi-child households, parents tend to allocate more resources toward older siblings. Younger siblings are more likely to drop out and enter the labor force, while older siblings are more likely to stay in school.

Our paper contributes to the literature exploring the effect of economic shock-induced

[3]The microfinance ban caused a 4.5% decrease in children's school enrollment in treated districts (column 1 in Table 1). According to the 2011 India Population Census, the population of children aged 5 to 19 in Andhra Pradesh was 23,599,744. Therefore, this effect translates to approximately 1.06 million children dropping out of school.

wage changes on human capital investment in children. Studies in developing countries have explored how child labor and schooling respond to rainfall shocks and document that human capital attainment is often counter-cyclical or exhibits seasonal fluctuation without any real effects on human capital (Shah and Steinberg, 2017; Jacoby and Skoufias, 1997).[4] Financial intermediation in the form of large asset transfers (Edmonds and Theoharides, 2020), expansion in agricultural credit (Hossain, 2023), and employment-based safety-net programs (Shah and Steinberg, 2021; Li and Sekhri, 2020) decrease human capital investment and increase child labor. On the other hand, several studies demonstrate that adverse shocks reduce human capital (Jensen, 2000; Beegle, Dehejia and Gatti, 2006; Duryea, Lam and Levison, 2007; Gubert and Robilliard, 2008; Björkman-Nyqvist, 2013). These repercussions are often construed as a test of credit market imperfections, as income shocks would not affect schooling if there were no financial market imperfections and households could borrow without constraints.[5]

We advance and complement this literature by demonstrating that credit constraints do not affect all children equally. Unlike these previous papers on credit expansion, we use a ban on microcredit as a source of variation to show that such shocks reduce enrollment, but there is significant heterogeneity in the effects across grades.[6] To the extent there are dynamic complementarities in education, and returns to education are higher for upper grades and degrees, households can be forward-looking and protect their investment in children in upper grades. Furthermore, our study presents a novel perspective on the impact of credit shocks by considering the unique setting of a microfinance ban, which contrasts with previous research on credit expansions. These findings have important policy implications, suggesting targeted interventions to safeguard younger children from dropping out of school and entering child labor markets, especially in times of credit crunch.

We also contribute to the literature on households' strategic investments in children. Households make reinforcing investments when endowments shift to improve cognitive functioning (Adhvaryu and Nyshadham, 2016; Venkataramani, 2012) and when shocks

[4]Counter-cyclicality is demonstrated in the U.S. as well. Cascio and Narayan (2022) demonstrate that economic booms from increased fracking in the United States increased high school dropout rates due to increased demand for unskilled labor.

[5]Poor and disadvantaged plausibly face similar circumstances in developed countries. Adverse economic shocks affect human capital for disadvantaged children in developed countries too (Coelli, 2011).

[6]Shah and Steinberg (2017) demonstrate heterogeneity in the impact of rainfall shocks on human capital by age. However, they find that positive rainfall shocks in utero to age two have a positive effect on human capital, but such shocks have a negative impact after age 5. This stems from time and income both being inputs in human capital production and older children possessing less time. In contrast, we find that among school-aged children, older ones are less likely to drop out, while younger children are more likely to drop out.

lower endowments (Akresh et al., 2012). Maternal education leads to compensating investments that offset negative birth endowments (Restrepo, 2016). Aizer and Cunha (2012) show that investments and existing human capital are complements, and due to dynamic complementarity, parents invest more in children with higher existing human capital or endowments. We contribute to this literature by showing that households also strategize to protect their investments in children in response to dynamic complementarity in education.

The remainder of this paper is structured as follows: Section 2 describes the study setting, while Section 3 details the data. Section 4 outlines the empirical methodology, and Section 5 presents the results. Section 6 explores potential mechanisms, followed by Section 7, which examines heterogeneity and conducts a robustness check. Finally, Section 8 concludes the paper.

2 Background

2.1 Child Labor in India

Child labor continues to be a pervasive issue in India. According to the International Labor Organization, 10.3 million children aged 5 to 14 were involved in child labor in South Asia in 2014, with the majority of these cases occurring in India (International Labour Organization, 2014). The 2011 Population Census of India notes that 10.1 million children in the same age range were working, with 64% of them employed as cultivators, agricultural laborers, or household industry workers (International Labour Organization, 2017). While the government strives to eliminate child labor, simply banning child labor can be counter-effective. For instance, Bharadwaj, Lakdawala and Li (2020) examine the Child Labor (Prohibition and Regulation) Act of 1986 and find that this policy actually increased the likelihood of child employment, especially among the poor. Meanwhile, Baland, Demont and Somanathan (2020) highlights that *microfinance* has the potential to improve school enrollment and reduce child labor in impoverished areas, emphasizing the important role it plays in rural India.

2.2 Microfinance in India

Microfinance plays a crucial role in India's economic development. It aims to foster financial inclusion for the poor and alleviate poverty. In 1992, the National Bank for Agriculture and Rural Development (NABARD) launched the first modern microfinance

program, the Self-Help Group-Bank Linkage Programme (SBLP). This program enables banks to directly finance Self-Help Groups (SHGs), with NABARD offering refinancing for these bank loans. Another major credit delivery modality is the Microfinance Institution (MFI), which grew rapidly in the late 1990s and the 2000s. MFIs are diverse, ranging from not-for-profit societies, trusts, and organizations to for-profit non-banking financial companies (NBFCs) and local banks. They are one of the most popular options for households to meet their credit needs as they provide access to credit without requiring collateral.

By March 2008, microfinance in India had reached an estimated 50 million individuals (45 million households). Around 36 million people were connected to the SBLP as savers, facilitating their SHGs in accessing bank loans. The remaining 14 million individuals were associated with MFIs, mainly as borrowers due to regulatory restrictions that prevented MFIs from offering deposit services.[7] The SBLP held a dominant position in offering savings services, with 35.6 million out of 38.4 million savers associated with it, while only 2.8 million were associated with MFIs. Conversely, in terms of borrowers, MFIs had a higher proportion: 11.9 million out of 19.1 million borrowers were MFI clients.

Microfinance services in India are primarily concentrated in South India, where the cultural and contextual dynamics, including the region's communal culture and welfare-oriented state policies, play pivotal roles in enhancing the reach of MFIs (Bhaskar, 2015; Saraf, 2015). To illustrate this, we provide a map of the distribution of MFIs across districts in 2008 in Figure 1a. This concentration is particularly notable in the southern regions. Using the 2011 Census of India, we examined whether districts without MFIs significantly differ from those with MFIs. As indicated in panel A of Appendix Table C.1, districts having MFIs generally have larger populations, more households, and a higher male demographic. Thus, in the following analysis, we only focus on districts with MFIs.

2.3 2010 Microfinance Crisis in Andhra Pradesh

Microfinance was ubiquitous in Andhra Pradesh (AP) in the late 2000s. About 85% of households in AP borrowed money from MFIs in 2009 (Ghiyazuddin and Gupta, 2012). By September 2010, MFIs in AP had over INR 72 billion in outstanding loans, with the average borrower's debt being $300, which was 45% higher than the average debt of a SBLP member (Sriram, 2012). At the same time, MFIs faced criticism for their nontransparent and high-interest rates, which ranged from 50% to 84%, and their coercive collection practices, such as doorstep loan management. The situation escalated in 2010 when over

[7] Data is from this report. For more details on the history of microfinance in India, see Sinha (2009).

80 individuals committed suicide after defaulting on micro-loans, prompting the state government to take action against MFIs (BBC News, 2010).

On October 15, 2010, the government of AP issued an emergency ordinance to regulate the money lending of MFIs in response to the increasing cases of committed suicides and public outcry in the state.[8] The ordinance brought a complete halt to MFI's activities. It prohibited the doorstep collection of loans and barred MFIs from giving new loans in Andhra Pradesh. As most borrowers ceased repayments, MFIs in AP found themselves unable to recover approximately INR 72 billion in outstanding loans. Consequently, banks grew reluctant to extend credit to MFIs in the state, fearing mass defaults.[9] It was only in recent years that MFIs re-entered AP and Telangana.[10]

3 Data

3.1 Data on Micro-Finance Institutions

We use two sources of data on MFIs. First, we compile a list of districts with at least one MFI from *The Bharat Microfinance Report: Quick Data 2008* compiled by Sa-Dhan, a Self-Regulatory Organization (SRO) for Microfinance Institutions appointed by the Central Bank of India. We display a map of districts with MFIs in Figure 1a, where the deep and light blue regions represent districts with MFIs. Second, we use the district-level exposure index developed by Breza and Kinnan (2021). This index quantifies the extent to which a district's loan portfolio was exposed to the crisis on the eve of the event. For our main empirical analysis, we first restrict the sample to districts with at least one MFI in 2008, resulting in 208 districts, and then further limit it to districts in AP and districts with zero exposure to the policy shock, yielding a final sample of 106 districts. Among these, 19 districts in AP constitute the treated group, while the remaining districts serve as a synthetic control group. The final sample distribution is shown in Figure 1b. As a robustness check, we relax the second restriction and include all districts with at least one MFI in 2008.

[8] For more details about the crisis: Sriram (2012); Mader (2013).

[9] Banks did become hesitant to lend money to MFIs in other states as well, expecting other states to pass regulatory measures. However, borrowers in these states consistently continued with their loan repayments, and the defaults did not spread beyond (Breza and Kinnan, 2021).

[10] Refer to this news article. Telangana was divided from Andhra Pradesh in 2014.

3.2 Data on Child Education

Our primary data source for assessing children's school enrollment status is the *Annual Status of Education Report* (ASER) survey. Conducted annually by the ASER center from 2005 to 2014, this survey reached households across all rural districts in India, collecting data on schooling outcomes such as enrollment, reading, and arithmetic levels for children aged 3 to 16.[11] The ASER survey has two key features: first, it provides a large and district-level representative sample, which is the unit of analysis in our main specification. Secondly, it includes all children in a household aged 3 to 16, regardless of their enrollment status. Our analysis uses survey data from 2007 to 2013, focusing on children aged over 4.[12] We then aggregate this data to create a district-level panel dataset.

In the data, we identify each child's grade level and then group students based on age to assess their likelihood of enrolling in age-appropriate schools. In India, the education system is typically structured into four levels: primary school (grades 1 to 5) for children under 10 years; middle school (grades 6 to 8) for ages 11 to 13; secondary school (grades 9 to 10) for the 14 to 16 age group; and higher secondary school (grades 11 and 12) for 16 to 17-year-olds. Specifically, in Section 5.1, we examine the likelihood of children aged 4 to 10 attending primary schools, those aged 11 to 13 attending middle schools, those aged 14 to 16 attending secondary schools, and those aged 14 to 16 attending higher secondary schools.[13]

3.3 Data on Child Labor

Our main data on labor market outcomes is sourced from the *National Sample Surveys* (NSS), conducted by the National Sample Survey Office (NSSO). These nationally representative household surveys collect various labor force indicators. The surveys also include district information on households, enabling us to aggregate the data at the district level. NSS Surveys are conducted in *rounds*, typically covering a one-year period, with some conducted over six months. For our study, we use data from the 61st (July 2004-June 2005), 62nd (July 2005-June 2006), 64th (July 2007-June 2008), 66th (July 2009-June 2010), and 68th (July 2011-June 2012) rounds of the Employment and Unemployment Survey. We focus on children aged 4 to 17 for two reasons: firstly, this age range typically in-

[11] For more information about the ASER survey sample, visit this website.

[12] In India, most primary schools admit children over the age of 5. For reference, see this article. Our findings remain consistent when varying the lower age limit between 3 to 5 years.

[13] For the last group, higher secondary schools, we relax the age restriction because the ASER survey only includes children aged up to 16. If we do not relax this restriction, we would only have data for children aged 16.

cludes those who have completed higher secondary school; secondly, the 2001 Census of India categorizes any economically productive activity by those under 17 as child labor.

3.4 Household-Level Panel Data

We also utilize the two waves of the *India Human Development Survey* (IHDS) to analyze individual-level outcomes, explore potential mechanisms, and conduct heterogeneity analyses. The IHDS is a nationally representative survey encompassing India's major states and union territories. The first wave, conducted in 2004-5, interviewed 41,554 households, and a follow-up wave in 2011-12 re-interviewed most of these households, totaling 42,152 households. The IHDS collected extensive information on various socio-demographic and economic outcomes.[14] We utilize data from these two survey waves along with census data to construct a panel of children who were below 11 years old in the first wave, ensuring they remained under 17 years old in the second wave. Our analysis includes a sample of 3,023 households and 5,896 school-aged children from 61 districts in India. Of these households, 70.3% were located in rural areas. The average number of school-age children in a household is 2.4, and the median is 2. About 23.7% of the households have only one school-aged child. In households with more than one school-aged child, the average number of school-aged children is 2.8.

4 Empirical Strategy

In this section, we describe our identification approach for estimating the effect of the Andhra Pradesh microcredit ban on children's schooling and labor force participation. As a first check, we compare district characteristics using data from the 2011 Population Census of India. Appendix Table C.1 presents these comparisons: Panel A compares districts with MFIs in other states to those with MFIs in AP, and Panel B examines zero-exposure districts with MFIs against districts with MFIs in AP. We find that, across all comparisons, districts in the former group consistently tend to have larger populations, a higher share of males, and a greater proportion of the working-age population. This suggests that our treated group (districts with MFIs in AP) differs significantly from our control group (i.e., zero-exposure districts with MFIs), underscoring the need to account for these differences in our empirical analysis.

To address this concern, we employ the Synthetic Difference-in-Differences strategy (SDID) proposed by Arkhangelsky et al. (2021). The method re-weights units in order to

[14] For a detailed description of this survey, see Desai et al. (2010).

match pre-treatment trends. It also allows for selection through unit-specific and time-specific fixed effects that are additive under the assumption of parallel trends. Under this approach, we compare the outcomes of exposed districts (treated group) with that of a weighted combination of zero-exposure districts (control group), which is constructed to have similar pre-treatment trends as the treated group. We use the unit weights and time weights derived from SDID to re-weigh our district panel data in the regressions. We use an R package developed by Arkhangelsky et al. (2021) to calculate the weights. Specifically, we match the districts based on a number of covariates.[15]

For a district i in year t, we then estimate the following regression specifications:

$$Y_{it} = \alpha_i + \alpha_t + \sum_{t=a}^{b} \beta_t \ \ Exposed_i \times 1(Year = t) + \varepsilon_{it} \tag{1}$$

$$Y_{it} = \alpha_i + \alpha_t + \beta \ \ Exposed_i \times Post_t + \varepsilon_{it} \tag{2}$$

where Y_{it} are a set of outcome variables of a district i in year t such as the proportion of surveyed children enrolled in school or engaged in different types of work. $Exposed_i$ is a time-invariant definition that takes the value 1 if a district is in Andhra Pradesh.

Equation (1) follows a standard event-study design, with $1(Year = t)$ assigned the value 1 in year t and 0 otherwise. We use 2009, the year preceding the halt of microfinance institutions' operations in Andhra Pradesh, as the base year. In this equation, a represents 2007 in the ASER data and 2004 in the NSSO data, while b corresponds to 2013 in the ASER data and 2012 in the NSSO data. In Equation (2), we combine the pre- and post-crisis years. Here, $Post_t$ is a variable that takes the value 1 for 2010-2013 in the ASER data (and for 2010-2012 in the NSSO data) and 0 for earlier years. α_i represents district fixed effects, accounting for all constant observed and unobserved district characteristics; α_t denotes year fixed effects. Since we construct the weights using a vector of district observables, we do not include these observables in our regression specification. Both district and year weights derived from the SDID method are applied in equations (1) and (2). Standard errors are clustered at the state level for statistical inference.

To ensure the robustness of our inference, we also directly implement the SDID method with three types of statistical inference: placebo, bootstrap, and jackknife. These inference

[15]For the ASER data, we match districts based on their number of households, number of mothers/fathers, number of mothers/fathers who belong to the Scheduled Caste or Tribes, number of mothers/fathers who are literate, number of mothers/fathers who are ill and could not work, numbers of mothers/fathers who work, the average number of household members, average mothers' age, average mothers' highest achieved education level, and share of mothers who have gone to schools. For the NSSO data, we match districts based on their share of households in the rural/urban area, the share of male/female, the average number of household members, average household annual income, the share of never-married women, the share of married women, and share of household belong to the Scheduled Caste or Tribes.

methods enhance the reliability of our estimates by providing robust standard errors.

5 Empirical Results

5.1 Impact on Child Education

We begin by examining the impact of the micro-finance ban on school enrollment among children aged 4 to 16 using the ASER data. The estimated coefficients $\hat{\beta}_t$ from the event-study specification, equation (1), are presented in Figure 2. As expected, the pre-period coefficients are small and not statistically significant.[16] Post-2010, however, we observe a negative and statistically significant divergence between the groups, persisting for four years. Districts in AP exhibit a 2.9-6.6 percentage point (p.p.) decrease in overall school enrollment compared to zero-exposure districts. We also show event study graphs by the level of schooling and age in Figure 3. Figures 3a, 3b, and 3c show a similar trend as in Figure 2 while Figure 3d presents an opposite post-period trend. Enrollment in early levels of schooling, including primary, middle, and secondary, drops significantly after the shock. In contrast, the impact on higher secondary schooling is positive and statistically significant.

Next, we report the results from equation (2) in Table 1. In columns 1-4, we observe a 4.5 p.p. reduction in the overall enrollment for children aged 4-16, a 3.9 p.p. drop in the primary school enrollment for children aged 4-10, a 5.2 p.p. decrease in the middle school enrollment for children aged 11-13, and a 7.1 p.p. decline in the secondary school enrollment for children aged 14-16. Given the pre-ban average overall school enrollment rate of 81 percent, this effect size translates to a reduction of 5.5% in the overall school enrollment. In column 5, we see a 3.2 p.p. increase in the higher secondary school enrollment for children aged 14-16. Furthermore, we present the child education results using the SDID method with three types of inference in Table 2. As shown, the magnitude and statistical significance of the estimated coefficients in Table 2 closely align with those in Table 1, suggesting that our results are robust.

As a robustness check, we also assess the impact on child education using data from both the NSSO and the IHDS. We present the estimated coefficients $\hat{\beta}$ using NSSO data in Appendix Table C.2, and using IHDS data in Appendix Table C.3. The results in both tables are similar to those in Table 1.[17]

[16] The event study graph on $\hat{\beta}_t$, shown in Appendix Figure D.1 without SDID weights, indicates the presence of pre-trends in the absence of reweighting.

[17] In Appendix Table C.3, $\hat{\beta}$ is positive for secondary school enrollment. This may be attributed to the fact that we do not restrict the IHDS data to a specific age group (i.e., ages 14 to 17).

5.2 Impact on Child Labor

We then explore the effects of microcredit shocks on labor market participation among children aged 4 to 17 years using data from the NSSO Employment and Unemployment surveys. We examine various working conditions, including self-employment (or working at household enterprises), wage/salary employment, domestic duties, and vocational training. We report the results of the estimation of equation (2) in panel A of Table 3. First, as shown in column 2, the micro-finance ban led to a statistically significant decrease in children aged 4-17 participating in self-employment activities. Children in policy-exposed areas are 1.3 p.p. less likely to engage in household enterprises compared to their counterparts in non-exposed districts. To the extent the micro-credit ban tightens the liquidity constraints of the exposed households, affecting capital flows in household enterprises, this result should not come as a surprise. Second, the effect of the shock on the likelihood that a child works in wage-earning activities is not statistically significant (column 3). However, we observe a statistically significant increase (0.9 p.p. or 19.3%) in the likelihood that children participate in domestic chores in the treated areas following the policy change (column 5).

Our investigation into the ban's effect on children's labor force participation by age group revealed particularly interesting results. As shown in panels B to D of Table 3, the reduction in the likelihood of employment in household enterprises is mainly driven by children in the age group of 14-17. Specifically, the probability of working in household enterprises for children in this age group is 2.4 p.p. lower (column 2 of panel D). The corresponding effect for the younger age group is economically negligible, though statistically significant (column 2 of panel B). With regard to wage-earning activities, we observe a different picture. While we find no effect overall of the credit ban on children's wage-earning activities (column 3 of panel A), we observe heterogeneity in the treatment effect. The likelihood of working for wage/salary increases significantly (2.1 p.p. or 16.8%) for younger children (column 3 of panel B) and decreases significantly (3.7 p.p. or 41.6%) for elder children (column 3 of panel D). Children aged 14-17 are also 1.7 p.p. (or 32.5%) less likely to go for vocational training, which prepares them for a specific occupation (column 4 of panel D).

Similarly, we present the results using the SDID method with three types of inference in Table 4. First, in terms of economic magnitudes, the estimated coefficients in Table 4 are identical to those in Table 3. Second, while the levels of statistical significance vary for some estimated coefficients, such as in columns 1 and 3 of panel B, across the three types of inference, the overall pattern remains consistent. The level of statistical significance in Table 4 is largely comparable to those in Table 3, reinforcing the robustness of our

findings.

6 Potential Mechanisms

Our results in Section 5 can be explained by two potential mechanisms: (1) dynamic complementarity and (2) strategic resource allocation by birth order. The first mechanism suggests that early investment in learning enhances the productivity of later investments, leading to increasing returns to education over time. Under credit constraints, this dynamic complementarity influences household decisions on school enrollment. During economic shocks, parents may prioritize the education of older children, who have already accumulated more human capital and thus yield higher returns from continued schooling. In contrast, younger children, whose educational investments have yet to generate significant returns, are more likely to drop out of school and enter the labor force to supplement household income. This forward-looking strategy reflects parental efforts to maximize long-term benefits in the face of financial limitations. Given that returns to education are higher at later stages (Colclough et al., 2010; Kijima, 2006; Schultz, 2004; Duflo, 2001), parents who have invested in their children's education through middle school may seek to protect this investment, potentially explaining our findings.[18]

An alternative possibility is that households allocate resources differently based on *birth order*. If parents invest more in the firstborn child, this child may be more resilient to economic shocks, and our results could reflect this effect. However, whether there is a consistently positive investment in firstborn children remains unclear. Previous research indicates that children with lower birth order are more likely to be breastfed longer (Jayachandran and Pande, 2017), yet they often receive less maternal time (Lindert, 1977; Birdsall, 1978) and fewer family resources (Behrman, 1988; Jayachandran and Pande, 2017). Hence, the evidence on the impact of birth order on educational outcomes is mixed.

To shed more light on this channel, we leverage and analyze education decisions in multi-child families with children in different age brackets. If larger investments in the first child were driving our results, then the specific grade the child is in would not be a determining factor, and older children would continue to be enrolled in school. We use the IHDS data to assess whether this is the case.

The IHDS data is a two-year household panel data set (2005 and 2012), so we employ a traditional Difference-in-Differences approach instead of the Synthetic Difference-in-

[18]For instance, Colclough et al. (2010) documents that, in India, returns to middle or lower secondary education is 7.2%, whereas the corresponding number for secondary or higher secondary education is 12.6%. Kijima (2006) showed that the returns to primary, secondary and higher education are 5.93%, 9.27%, and 11%, respectively.

Differences method discussed in Section 4. The analysis proceeds as follows: First, we replicate our main findings using this data. Then, we show that younger children are more likely to drop out of school and work in treated districts than older ones. After this, we leverage the ages of siblings and their grade levels to differentiate the *birth order effect* from the *investment protection effect*.

To empirically estimate the impact of the microcredit ban on child j, we employ the following specification:

$$Y_{jr} = \alpha_j + \alpha_r + \beta \ \ Exposed_j \times Post_r + \gamma X_{jr} + \epsilon_{jt} \tag{3}$$

where Y_{jt} are a set of outcome variables of a child j in survey round r such as school enrollment status, working status, and working hours. $Exposed_j$ is a time-invariant indicator set to 1 for children in districts with micro-finance institutions in Andhra Pradesh. $Post_r$ is assigned the value 1 for the second survey round in 2011-12 and 0 for the first round in 2004-05. α_j represents individual fixed effects, controlling for all constant observed and unobserved characteristics of a child, such as academic abilities, family support, etc. α_r denotes survey round fixed effects. X_{jr} includes a range of control variables: the education level and employment status of the child's parents; the household's urban or rural status; land and livestock ownership; and the household's total assets, annual consumption, and income.

The results replicating our main findings in the IHDS data are reported in Table 5. Column 1 of panel A in Table Table 5 shows that the microfinance ban negatively affected (-8.6%) school enrollment for children in the treated districts. At the extensive margin, columns 2 to 5 in panel A reveal an overall reduction in labor participation, primarily driven by decreases in farming and animal care activities. However, more children work for wages or salaries in the treated districts than in the control districts.[19] Panel B indicates a similar pattern regarding the intensive margin of working hours. Although total working hours decreased, children in treated districts spent more time on wage/salary activities after the ban. These findings reinforce that our main empirical findings are robust in the IHDS data. Finding similar results using two independent datasets bolsters our main findings.

After establishing that our main empirical findings also bear out in the IHDS data, we now focus on households with multiple children.[20] We first explore the heterogeneity in

[19]This is consistent with the results in Table 3, which show a decrease in self-employment activities, including farming and non-farming business work, for children in the treated districts during the post-ban period.

[20]Although we focus on households with multiple children, we also replicate the analysis from panel

the treatment effect based on the *birth order* of the children in these families. We define "Younger Sibling" as a dummy variable that takes a value of one if the child *has at least one elder sibling* and zero otherwise. We report the results in Table 6. We find that the negative effect on school enrollment is driven by younger siblings. Specifically, after the MFI ban, school enrollment for younger siblings is 6.6% lower than for older siblings. A similar pattern emerges for child labor, though the effects are not statistically significant at conventional levels: younger siblings in treated districts are 4.4% more likely to engage in any form of child labor during the post-ban period.

Next, we focus on children's age and school grades. We categorize households into four groups: (i) households only having kids aged less than 4 in 2005 (so that the children are 11 or below in 2012) enrolled in primary school grades in 2012,[21] (ii) households only having kids aged between 4 and 7 in 2005 (11 and 14 in 2012) enrolled in middle school grades, (iii) households only having kids aged more than 7 in 2005 (older than 14 in 2012) enrolled in senior and senior secondary school grades, and (iv) households having one kid aged more than 7 (older than 12) and one kid aged less than 4 in 2005 (aged 9 or below in 2012).

Table 7 presents estimates from our main specification on four sub-samples. Panel A shows that among siblings aged 11 or below in 2012, who are of primary school-going age, the pattern reverses—the younger child remains enrolled in school, with an estimated coefficient of 11.3% and a significance level of $p < 0.15$. If birth-order investments were driving our results, older children would be more likely to remain in school even within this age group. However, we observe the opposite pattern among primary school-aged children. In panels B and C, where both siblings are old enough to contribute to household income, those in higher grades remain in school, while younger siblings increase their labor supply. Comparing panels A and D, where one sibling is potentially in primary school and the other in high school, we find results consistent with our main findings: younger siblings are less likely to enroll in school and more likely to work (columns 2 and 5 in panel D). Taken together, these patterns, which change with sibling age, cannot be fully explained by birth-order effects alone. Instead, the evidence weighs in favor of the idea that households prioritize protecting their investment in older children's education.

A of Table 5 for households with only one school-aged child. We report the results for these children in Appendix Table C.4. As shown, the signs of the estimated coefficients are in the same direction as those in panel A of Table 5.

[21] For example, in the data, 95% of the children belonging to this group were either enrolled in the primary schools or not enrolled at all in 2012. Only 15 kids skipped grades and went to middle school in 2012.

7 Heterogeneity Analysis & Robustness

7.1 Heterogeneity Analysis

We also take advantage of individual-level data from the IHDS to examine the heterogeneity in the treatment effect across different groups, focusing on gender, household wealth status (i.e., below or above the poverty line), and caste. The regression results are presented in Appendix Table C.5, Table C.6, and Table C.7, respectively. Firstly, our analysis reveals no significant gender differences in school enrollment (column 1 of Appendix Table C.5) or child labor (columns 2-5 of Appendix Table C.5). Secondly, children from poorer families tend to spend less time on farming or animal care, as these households often lack the necessary resources (column 3 of Appendix Table C.6). Thirdly, children from lower castes find themselves in less favorable work environments and consequently have fewer opportunities to work and contribute financially to their families (column 5 of Appendix Table C.7). These findings highlight how household wealth and caste status mediate the impact of the microfinance ban. The limited work opportunities for lower-caste children and resource constraints faced by poorer households highlight existing structural inequalities, potentially reinforcing long-term economic disadvantages.

7.2 Robustness Check

We perform a robustness check to validate our findings in Section 5 and Section 6. In the main analysis, we restrict the control group to non-exposure districts only. Here, we expand the control group to include all districts that had at least one MFI in 2008, allowing us to assess whether our results hold with a larger sample size. We present the results in Appendix A. As shown, both the estimated coefficients and their statistical significance levels remain consistent, providing additional support for the robustness of our findings.

8 Conclusion

The effect of wage shocks on human capital has been the subject of extensive literature with mixed findings. Theoretically, the effect can go in either direction depending on the relative strength of the substitution and income effect. In this paper, we revisit the debate leveraging a unique shock through a massive contraction in credit supply. In 2010, an ordinance by the state of Andhra Pradesh, India, completely halted the operations of all the private microfinance organizations in the state, adversely affecting their loan recovery

and liquidity. We exploit the variation in the credit supply generated by this natural experiment to explore the effect of a reduction in credit supply on children's human capital attainment and labor supply.

We use a Synthetic Difference-in-difference strategy to show that a reduction in microcredit access lowered school enrollment by six percentage points, highlighting a dominant income effect. Therefore, our results are consistent with the strand of literature that finds positive income shocks lead to more investment in human capital (Jacoby and Skoufias, 1997; Jensen, 2000; Thomas, Beegle, Frankenberg, Sikoki, Strauss and Teruel, 2004; Maccini and Yang, 2009), and contradicts the literature documenting counter-cyclicality in human capital investment due to a dominant substitution effect (Hossain, 2023; Shah and Steinberg, 2021; Li and Sekhri, 2020; Edmonds and Theoharides, 2020; Shah and Steinberg, 2021). However, our paper differs from all the papers examining the impact of wages on human capital in that we investigate the effect of a *credit contraction*. At the same time, all prior studies focused on improving labor market conditions. The impact of increasing or contracting credit access can be asymmetric due to various factors (Breza and Kinnan, 2021). Thus, we fill a void in the literature by examining the effect of credit contraction on human capital. We also find enduring impacts that last beyond the year immediately following the credit ban. This implies that the younger children who do not enroll due to the ban have a lasting effect on their human capital acquisition.

The central contribution of the paper is that we show that the effect of credit access on human capital is not homogeneous across all children. We theorize that this effect is not homogeneous for all children due to the dynamic complementary in education, where early educational investment in human capital makes the current investment more productive. In families with multiple children, parents might strategize and make different decisions for the younger and older ones. Indeed, we find that the adverse shock affects younger children in the early years of schooling. Younger children in lower grades are less likely to continue to enroll in schools and more likely to participate in wage-earning activities. In contrast, older children in higher grades stay in school and do not work more to subsidize family income.

Overall, our findings indicate that in the presence of dynamic complementarities, the opportunity cost of schooling varies among children of different age groups and grade levels. Thus, the effect of the opportunity cost of schooling on human capital is not homogeneous for all children. Our findings have significant implications for policy design. Policy programs designed to protect children from dropping out of school during economic hardship should first target younger children of school-going age, especially if resources are scarce.

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Tables

TABLE 1: District-Level Enrollment by Age and Education Level (ASER)

	(1)	(2)	(3)	(4)	(5)
Dependent Variable: Proportion of Children within a Specific Age Range Enrolled in Schools					
Education Level	Overall	Primary	Middle	Secondary	Higher Secondary
Age range	(4∼16)	(4∼10)	(11∼13)	(14∼16)	(14∼16)
Exposed districts × Post MFI ban	-0.045**	-0.039***	-0.052***	-0.071***	0.032***
	(0.018)	(0.006)	(0.014)	(0.012)	(0.005)
District FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Control Mean	0.81	0.75	0.63	0.44	0.07
R^2	0.63	0.60	0.82	0.76	0.76
N	693	693	693	693	693

Notes: We aggregated the ASER data at the district level, focusing on specific age groups: 4 to 16, 4 to 10, 11 to 13, and 14 to 16 years. The sample includes zero-exposure districts and districts in Andhra Pradesh. The dependent variable is the proportion of children enrolled in school within each district. Column 1 presents overall school enrollment, encompassing primary, middle, secondary, and higher secondary schools for children aged 4 to 16. Column 2 details primary school enrollment for children aged 4 to 10, while Column 3 covers middle school enrollment for those aged 11 to 13. Columns 4 and 5 report secondary and higher secondary school enrollments, respectively, for children aged 14 to 16. All regressions include district and year fixed effects, with standard errors clustered at the state level. District weights from the synthetic difference-in-difference method are applied. Significance levels are indicated as follows: + p$<$ 0.15, * p $<$ 0.1, ** p $<$ 0.05, *** p $<$ 0.01.

TABLE 2: District-Level Enrollment by Age and Education Level (ASER): SDID with Different Inference Methods

	(1)	(2)	(3)	(4)	(5)
Dependent Variable: Proportion of Children within a Specific Age Range Enrolled in Schools					
Education Level	Overall	Primary	Middle	Secondary	Higher Secondary
Age range	(4∼16)	(4∼10)	(11∼13)	(14∼16)	(14∼16)
Panel A. Bootstrap Inference					
Exposed districts × Post MFI ban	-0.047***	-0.039***	-0.052***	-0.071***	0.032***
	(0.013)	(0.010)	(0.015)	(0.013)	(0.009)
Panel B. Jackknife Inference					
Exposed districts × Post MFI ban	-0.047***	-0.039***	-0.052***	-0.071***	0.032***
	(0.013)	(0.010)	(0.013)	(0.013)	(0.008)
Panel C. Placebo Inference					
Exposed districts × Post MFI ban	-0.047***	-0.039***	-0.052***	-0.071***	0.032***
	(0.014)	(0.009)	(0.014)	(0.016)	(0.008)
N	693	693	693	693	693

Notes: We aggregated the ASER data at the district level, focusing on specific age groups: 4 to 16, 4 to 10, 11 to 13, and 14 to 16 years. The sample includes zero-exposure districts and districts in Andhra Pradesh. The dependent variable is the proportion of children enrolled in school within each district. Column 1 presents overall school enrollment, encompassing primary, middle, secondary, and higher secondary schools for children aged 4 to 16. Column 2 details primary school enrollment for children aged 4 to 10, while Column 3 covers middle school enrollment for those aged 11 to 13. Columns 4 and 5 report secondary and higher secondary school enrollments, respectively, for children aged 14 to 16. We employ the synthetic difference-in-differences method on district-year panel data. Standard errors are reported using bootstrap inference with 100 repetitions in Panel A, jackknife inference in Panel B, and placebo inference with 100 repetitions in Panel C. Significance levels are indicated as follows: + p$<$ 0.15, * p $<$ 0.1, ** p $<$ 0.05, *** p $<$ 0.01.

TABLE 3: Working Conditions at the District Level by Age (NSSO)

	(1)	(2)	(3)	(4)	(5)
		Work		Vocational	Domestic
	Any	Self-Employment	Wage/Salary	Training	Duties
Panel A. All children (4∼17)					
Exposed districts x Post MFI ban	-0.007	-0.013***	0.004	-0.002	0.009***
	(0.008)	(0.002)	(0.008)	(0.002)	(0.002)
Pre-ban Control Mean	0.13	0.03	0.10	0.05	0.05
R^2	0.63	0.52	0.61	0.52	0.46
Panel B. Age between 4 and 10					
Exposed districts x Post MFI ban	0.019^{+}	-0.002***	0.021*		-0.000
	(0.011)	(0.000)	(0.011)		(0.001)
Pre-ban Control Mean	0.13	0.00	0.13		0.01
R^2	0.61	0.26	0.61		0.23
Panel C. Age between 11 and 13					
Exposed districts x Post MFI ban	-0.012*	-0.013***	0.001		0.009***
	(0.006)	(0.002)	(0.006)		(0.002)
Pre-ban Control Mean	0.07	0.02	0.06		0.04
R^2	0.47	0.43	0.38		0.31
Panel D. Age between 14 and 17					
Exposed districts x Post MFI ban	-0.066***	-0.024***	-0.037***	-0.017**	0.025***
	(0.008)	(0.004)	(0.008)	(0.006)	(0.007)
Pre-ban Control Mean	0.18	0.09	0.09	0.05	0.13
R^2	0.65	0.44	0.54	0.52	0.41
District FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	1,197	1,197	1,197	1,197	1,197

Notes: We aggregated the NSSO data at the district level. For children under 14, we lack observations on vocational training, precluding estimates of its impacts. In panel A, prior to aggregation, we retained observations of children aged 4 to 17. For panels B to D, we similarly prepared the data before aggregation, focusing on specific age groups: 4 to 10, 11 to 13, and 14 to 17 years. The sample includes zero-exposure districts and districts in Andhra Pradesh. "Any" = The portion of children who participate in self-employment activities or work for wage/salary. "Self-Employment" = The portion of children who work in the household enterprise (self-employment). "Wage/Salary" = The portion of children who work at non-household enterprises. "Vocational Training" = The portion of children who receive any form of vocational training, including formal vocational training, non-formal vocational training, self-learning, learning on the job, and others. "Domestic Duties" = The portion of children who engage in domestic duties only. All regressions include district and year fixed effects, with standard errors clustered at the state level. District weights from the synthetic difference-in-difference method are applied. Significance levels are indicated as follows: + p$<$ 0.15, * p $<$ 0.1, ** p $<$ 0.05, *** p $<$ 0.01.

TABLE 4: Working Conditions at the District Level by Age (NSSO): SDID with Different Inference Methods

	(1)	(2) Work	(3)	(4) Vocational	(5) Domestic
	Any	Self-Employment	Wage/Salary	Training	Duties
Panel A. All children (4∼17)					
Exposed districts x Post MFI ban	-0.007	-0.012	0.004	-0.002	0.009
Bootstrap SE	(0.008)	(0.004)***	(0.007)	(0.004)	(0.004)**
Jackknife SE	(0.007)	(0.003)***	(0.006)	(0.003)	(0.004)**
Placebo SE	(0.008)	(0.003)***	(0.014)	(0.003)	(0.005)*
Panel B. Age between 4 and 10					
Exposed districts x Post MFI ban	0.019	-0.002	0.021		-0.000
Bootstrap SE	(0.007)***	(0.001)	(0.007)***		(0.002)
Jackknife SE	(0.007)***	(0.001)*	(0.007)***		(0.000)
Placebo SE	(0.016)	(0.001)*	(0.016)		(0.002)
Panel C. Age between 11 and 13					
Exposed districts x Post MFI ban	-0.012	-0.013	0.001		0.009
Bootstrap SE	(0.011)	(0.005)***	(0.009)		(0.005)*
Jackknife SE	(0.009)	(0.005)***	(0.009)		(0.005)*
Placebo SE	(0.012)	(0.006)**	(0.012)		(0.008)
Panel D. Age between 14 and 17					
Exposed districts x Post MFI ban	-0.066	-0.024	-0.037	-0.017	0.025
Bootstrap SE	(0.012)***	(0.009)***	(0.011)***	$(0.011)^{+}$	(0.012)**
Jackknife SE	(0.011)***	(0.008)***	(0.011)***	$(0.011)^{+}$	(0.011)**
Placebo SE	(0.014)***	(0.010)**	(0.014)***	(0.009)*	(0.014)*
N	1,197	1,197	1,197	1,197	1,197

Notes: We aggregated the NSSO data at the district level. For children under 14, we lack observations on vocational training, precluding estimates of its impacts. In panel A, prior to aggregation, we retained observations of children aged 4 to 17. For panels B to D, we similarly prepared the data before aggregation, focusing on specific age groups: 4 to 10, 11 to 13, and 14 to 17 years. The sample includes zero-exposure districts and districts in Andhra Pradesh. "Any" = The portion of children who participate in self-employment activities or work for wage/salary. "Self-Employment" = The portion of children who work in the household enterprise (self-employment). "Wage/Salary" = The portion of children who work at non-household enterprises. "Vocational Training" = The portion of children who receive any form of vocational training, including formal vocational training, non-formal vocational training, self-learning, learning on the job, and others. "Domestic Duties" = The portion of children who engage in domestic duties only. We employ the synthetic difference-in-differences method on district-year panel data. We report standard errors estimated using different inference methods (bootstrap, jackknife, and placebo) in the parentheses. Significance levels are indicated as follows: + $p< 0.15$, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 5: Enrollment & Employment (IHDS): Individual Level

	(1)	(2)	(3)	(4)	(5)
Panel A. Overall Enrollment & Employment					
	School	Work			
	Enrollment	Any	Farm/Animal	Business	Wage/Salary
Exposed districts × Post MFI ban	-0.086***	-0.079**	-0.096***	0.000	0.024**
	(0.028)	(0.035)	(0.034)	(0.003)	(0.010)
Pre-ban Control Mean	0.53	0.02	0.02	0.00	0.00
R^2	0.58	0.61	0.61	0.51	0.53
Panel B. Working Hours					
		Ln(Work Hours)	Ln(Farm Hours)	Ln(Business Hours)	Ln(Wage/Salary Hours)
Exposed districts × Post MFI ban		-0.216*	-0.329***	0.002	0.155**
		(0.120)	(0.104)	(0.016)	(0.075)
Pre-ban Control Mean		0.05	0.04	0.01	0.00
R^2		0.58	0.59	0.50	0.53
Individual FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	9,174	9,174	9,174	9,174	9,174

Notes: We restrict our IHDS sample to kids who are less than or equal to 11 years old in the 2005 survey. The sample includes zero-exposure districts and districts in Andhra Pradesh. "School Enrollment" = A dummy variable equal to one if a child is enrolled at a school (e.g., primary school, middle school, secondary school, and higher secondary school) currently and zero otherwise. "Any" = A dummy variable equal to one if a child does farm/animal care work, non-farm business work, or wage/salary work and zero otherwise. "Farm/Animal" = A dummy variable equal to one if a child does farm or animal care work and zero otherwise. "Business" = A dummy variable equal to one if a child does non-farm business work and zero otherwise. "Wage/Salary" = A dummy variable equal to one if a child does wage or salary work and zero otherwise. "Work Hours" include hours of working in farming, animal caring, business, and wage/salary work. "Farm Hours" include hours spent on farm and/or animal care work. "Business Hours" include hours spent on non-farm work. "Wage/Salary Hours" include hours spent on wage or salary work. We take the inverse hyperbolic transformation of the hour-related variables to retain the zeros. All regressions include individual and year fixed effects. We also control for various factors, including whether a household owned or cultivated land, whether a household owned livestock, the household's total assets, whether the father or mother is literate, whether the father or mother has attended school, the father's or mother's completed years of education, whether the father or mother is working, and whether the father or mother has any major morbidity, such as heart disease, cancer, or diabetes. Standard errors are clustered at the district level. Significance levels are indicated as follows: + p< 0.15, * p < 0.1, ** p < 0.05, *** p < 0.01.

TABLE 6: Employment (IHDS): Households with *Multiple* School-Aged Children

	(1) School	(2)	(3)	(4)	(5)
		Work			
	Enrollment	Any	Farm/Animal	Business	Wage/Salary
Exposed districts × Post MFI ban	0.001	-0.146***	-0.134***	-0.021*	-0.018
	(0.041)	(0.042)	(0.042)	(0.012)	(0.026)
Exposed districts × Post MFI ban	-0.066	0.044^{+}	0.030	0.007	0.018
× Younger Sibling	(0.054)	(0.030)	(0.023)	(0.007)	(0.026)
Individual FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Pre-ban Control Mean of Elder Sibling	0.74	0.06	0.05	0.00	0.00
R^2	0.47	0.48	0.48	0.34	0.32
N	7,550	7,550	7,550	7,550	7,550

Notes: We restrict our IHDS sample to kids who are less than or equal to 11 years old and have siblings aged less than or equal to 11 years old in the 2005 survey. "School Enrollment" = A dummy variable equal to one if a child is enrolled at a school (e.g., primary school, middle school, secondary school, and higher secondary school) currently and zero otherwise. "Any" = A dummy variable equal to one if a child does farm/animal care work, non-farm business work, or wage/salary work and zero otherwise. "Farm/Animal" = A dummy variable equal to one if a child does farm or animal care work and zero otherwise. "Business" = A dummy variable equal to one if a child does non-farm business work and zero otherwise. "Wage/Salary" = A dummy variable equal to one if a child does wage or salary work and zero otherwise. "Younger Sibling" = A dummy variable equal to one if the child has older siblings who aged less than or equal to 11 years old in the 2005 survey and zero if the child does not have an elder sibling who aged less than or equal to 11 years old in the 2005 survey. All regressions include individual and year fixed effects. We also control for various factors, including whether a household owned or cultivated land, whether a household owned livestock, the household's total assets, whether the father or mother is literate, whether the father or mother has attended school, the father's or mother's completed years of education, whether the father or mother is working, and whether the father or mother has any major morbidity, such as heart disease, cancer, or diabetes. Standard errors are clustered at the district level. Significance levels are indicated as follows: + $p< 0.15$, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

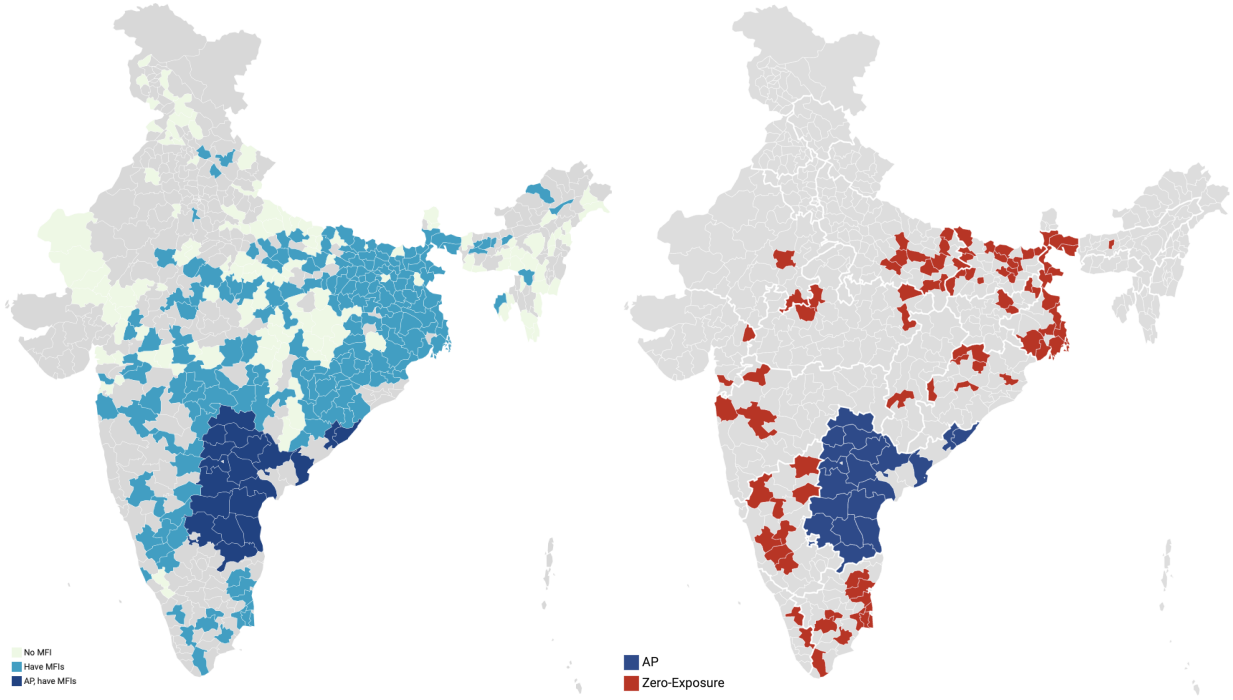
TABLE 7: Employment (IHDS): Households with *Multiple* School-Aged Children (By Education)

	(1)	(2)	(3)	(4)	(5)
	School	Work			
	Enrollment	Any	Farm/Animal	Business	Wage/Salary
Panel A. All kids' age $<$ 4 in 2005-06 (Primary School)					
Exposed districts × Post MFI ban	-0.053	-0.092^{+}	-0.111*	0.003	
	(0.085)	(0.059)	(0.056)	(0.025)	
Exposed districts × Post MFI ban	0.113^{+}	0.038	0.055*	-0.002	
× Younger Sibling	(0.075)	(0.036)	(0.032)	(0.022)	
Pre-ban Control Mean	0.03	0.02	0.02	0.00	
R^2	0.95	0.69	0.70	0.55	
N	524	524	524	524	
Panel B. All kids' age $\in [4,7]$ in 2005-06 (Middle School)					
Exposed districts × Post MFI ban	0.122^{+}	-0.239***	-0.257***	-0.000	-0.013
	(0.076)	(0.078)	(0.074)	(0.035)	(0.036)
Exposed districts × Post MFI ban	-0.263**	0.145**	0.164**	0.008	-0.024
× Younger Sibling	(0.099)	(0.067)	(0.068)	(0.023)	(0.036)
Pre-ban Control Mean	0.85	0.04	0.02	0.01	0.00
R^2	0.66	0.66	0.68	0.55	0.56
N	672	672	672	672	672
Panel C. All kids' age $>$ 7 in 2005-06 (Secondary and Higher Secondary School)					
Exposed districts × Post MFI ban	0.076	-0.159^{+}	-0.090	-0.056**	0.050
	(0.068)	(0.103)	(0.099)	(0.024)	(0.091)
Exposed districts × Post MFI ban	-0.176*	0.096	-0.026	0.024	-0.008
× Younger Sibling	(0.098)	(0.114)	(0.075)	(0.033)	(0.114)
Pre-ban Control Mean	0.94	0.06	0.05	0.00	0.01
R^2	0.72	0.69	0.70	0.62	0.60
N	472	472	472	472	472
Panel D. One kid's age $<$ 4 and One kid's age $>$ 7 in 2005-06					
Exposed districts × Post MFI ban	0.032	-0.321***	-0.200**	-0.012	-0.200***
	(0.078)	(0.107)	(0.098)	(0.009)	(0.070)
Exposed districts × Post MFI ban	-0.099	0.104*	0.033	-0.007	0.163***
× Younger Sibling	(0.122)	(0.057)	(0.049)	(0.008)	(0.052)
Pre-ban Control Mean	0.88	0.09	0.08	0.02	0.00
R^2	0.79	0.70	0.68	0.67	0.64
N	612	612	612	612	612
Individual FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Notes: We restrict our IHDS sample to children who are 11 years old or younger and have siblings within the same age range as of the 2005 survey. In panel A, the sample is confined to children from households with only children under 4 years old in 2005. For panel B, the sample is limited to children in households with only children aged 4 to 7 years in 2005. In panel C, we focus on children in households with only children older than 7 years in 2005. Panel D's sample comprises children from households with at least one child under 4 years old and at least one child over 7 years old in 2005. "School Enrollment" = A dummy variable equal to one if a child is enrolled at a school (e.g., primary school, middle school, secondary school, and higher secondary school) currently and zero otherwise. "Any" = A dummy variable equal to one if a child does farm/animal care work, non-farm business work, or wage/salary work and zero otherwise. "Farm/Animal" = A dummy variable equal to one if a child does farm or animal care work and zero otherwise. "Business" = A dummy variable equal to one if a child does non-farm business work and zero otherwise. "Wage/Salary" = A dummy variable equal to one if a child does wage or salary work and zero otherwise. "Younger Sibling" = A dummy variable equal to one if the child has older siblings who aged less than or equal to 11 years old in the 2005 survey and zero if the child does not have an elder sibling who aged less than or equal to 11 years old in the 2005 survey. All regressions include individual and year fixed effects. We also control for various factors, including whether a household owned or cultivated land, whether a household owned livestock, the household's total assets, whether the father or mother is literate, whether the father or mother has attended school, the father's or mother's completed years of education, whether the father or mother is working, and whether the father or mother has any major morbidity, such as heart disease, cancer, or diabetes. Standard errors are clustered at the district level. Significance levels are indicated as follows: + p$<$ 0.15, * p $<$ 0.1, ** p $<$ 0.05, *** p $<$ 0.01.

Figures

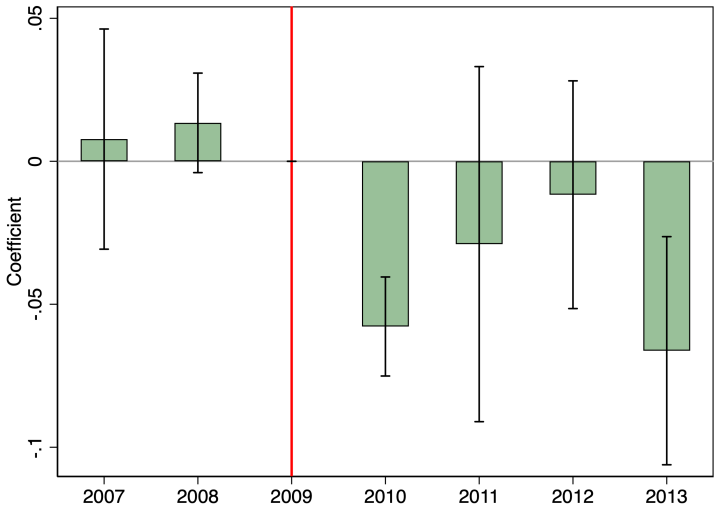
FIGURE 1: Map of Districts



(a) Distribution of MFIs across Districts (2008) (b) Zero-Exposure Districts & Andhra Pradesh

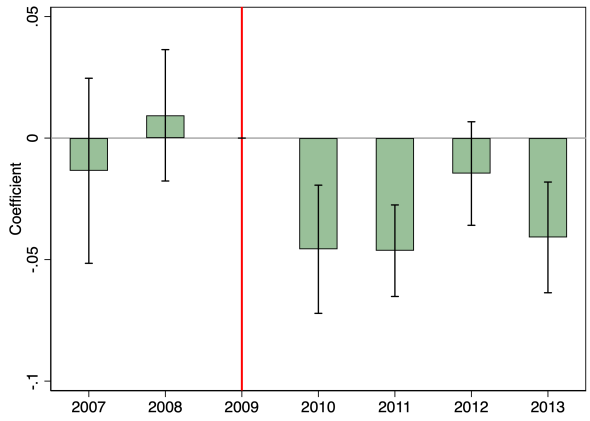
Notes: In Figure 1a, we plot the spatial distribution of microfinance institutions (MFIs) across districts. The data we used is from *The Bharat Microfinance Report: Quick Data 2008*. Gray areas: we do not have information on these districts. Light green areas: Districts without any MFIs in 2008. Light blue areas: Districts had at least one MFI in 2008. Dark blue area: Districts located in Andhra Pradesh that had at least one MFI in 2008. In Figure 1b, we plot the spatial distribution of zero-exposure districts using the index developed by Breza and Kinnan (2021), along with districts in Andhra Pradesh. Red areas: Districts with a zero exposure index that had at least one MFI in 2008. Dark blue area: Districts located in Andhra Pradesh that had at least one MFI in 2008.

FIGURE 2: Event Study Graph: District-Level Enrollment (ASER)

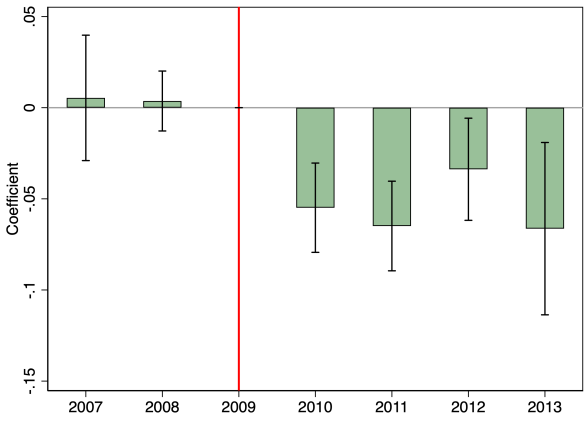


Notes: We collapsed the ASER data at the district level. Before collapsing the data, we kept observations aged between 4 and 16. The dependent variable is the proportion of children enrolled in school within a district. Column 1 is the overall school enrollment, which includes primary school, middle school, secondary school, and higher secondary school, among children aged between 4 and 16. The regression includes district and year fixed effects. Standard errors are clustered at the state level. District and year weights from synthetic difference-in-difference are applied. Confidence intervals are at the 95 percent level.

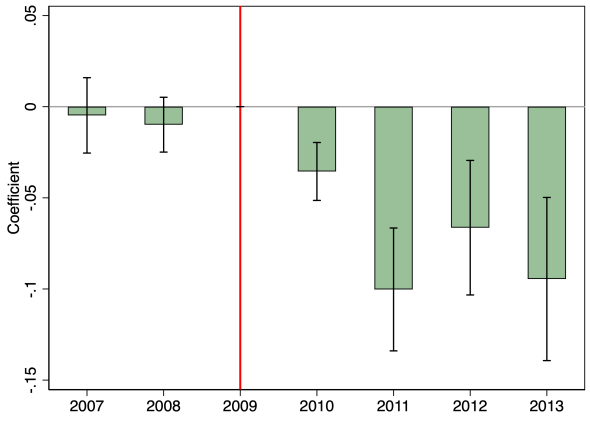
FIGURE 3: Event Study Graphs: District-Level Enrollment by Age Groups (ASER)



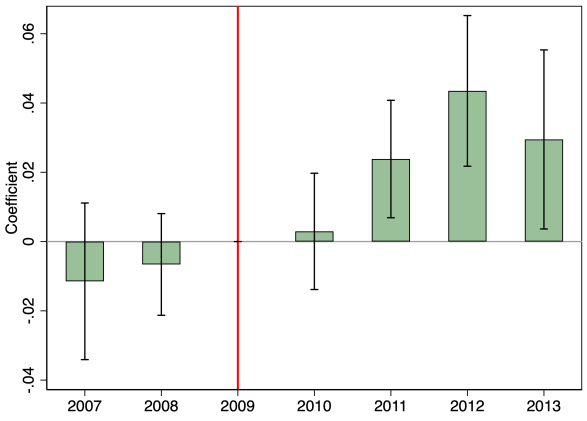
(a) Primary Schools (Age 4∼10)



(b) Middle Schools (Age 11∼13)



(c) Secondary Schools (Age 14∼16)



(d) Higher Secondary Schools (Age 14∼16)

Notes: We collapsed the ASER data at the district level. Before collapsing the data, we kept observations belonging to a certain age group (e.g. age between 4 and 16, age between 4 to 10, age between 11 and 13, and age between 14 and 16). The dependent variable in Figure 3a is the proportion of children aged between 4 and 10 who are enrolled in the primary schools within a district. The dependent variable in Figure 3b is the proportion of children aged between 11 and 13 who are enrolled in the middle schools within a district. The dependent variable in Figure 3c is the proportion of children aged between 14 and 16 who are enrolled in secondary schools within a district. The dependent variable in Figure 3d is the proportion of children aged between 14 and 16 who are enrolled in the higher secondary schools within a district. All regressions include district and year fixed effects. Standard errors are clustered at the state level. District and year weights from synthetic difference-in-difference are applied. Confidence intervals are at the 95 percent level.

APPENDIX

A Robustness Check

TABLE A1: District-Level Enrollment by Age and Education Level (ASER, AP vs. Non-AP Districts)

	(1)	(2)	(3)	(4)	(5)
Dependent Variable: Proportion of Children within a Specific Age Range Enrolled in Schools					
Education Level	Overall	Primary	Middle	Secondary	Higher Secondary
Age range	(4∼16)	(4∼10)	(11∼13)	(14∼16)	(14∼16)
Exposed districts × Post MFI ban	-0.060***	-0.043***	-0.055***	-0.078***	0.033***
	(0.010)	(0.009)	(0.007)	(0.009)	(0.005)
District FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Pre-ban control mean	0.80	0.74	0.61	0.43	0.05
R^2	0.64	0.57	0.75	0.75	0.74
N	1,427	1,427	1,427	1,427	1,427

Notes: We restrict the sample to districts with at least one MFI in 2008. We aggregated the ASER data at the district level, focusing on specific age groups: 4 to 16, 4 to 10, 11 to 13, and 14 to 16 years. The dependent variable is the proportion of children enrolled in school within each district. Column 1 presents overall school enrollment, encompassing primary, middle, secondary, and higher secondary schools for children aged 4 to 16. Column 2 details primary school enrollment for children aged 4 to 10, while Column 3 covers middle school enrollment for those aged 11 to 13. Columns 4 and 5 report secondary and higher secondary school enrollments, respectively, for children aged 14 to 16. All regressions include district and year fixed effects, with standard errors clustered at the state level. District weights from the synthetic difference-in-difference method are applied. Significance levels are indicated as follows: + p$<$ 0.15, * p $<$ 0.1, ** p $<$ 0.05, *** p $<$ 0.01.

TABLE A2: Working Conditions at the District Level by Age (NSSO, AP vs. Non-AP Districts)

	(1)	(2)	(3)	(4)	(5)
		Work		Vocational	Domestic
	Any	Self-Employment	Wage/Salary	Training	Duties
Panel A. All children (4∼17)					
Exposed districts x Post MFI ban	-0.004	-0.010***	0.008	-0.006***	0.005**
	(0.008)	(0.002)	(0.008)	(0.001)	(0.002)
Pre-ban Control Mean	0.14	0.03	0.11	0.04	0.04
R^2	0.64	0.45	0.61	0.49	0.41
Panel B. Age between 4 and 10					
Exposed districts x Post MFI ban	0.022*	-0.001	0.026*		-0.001
	(0.010)	(0.001)	(0.013)		(0.001)
Pre-ban Control Mean	0.13	0.00	0.13		0.01
R^2	0.59	0.26	0.59		0.21
Panel C. Age between 11 and 13					
Exposed districts x Post MFI ban	-0.010	-0.005	0.001		0.010**
	(0.007)	(0.003)	(0.006)		(0.004)
Pre-ban Control Mean	0.08	0.02	0.06		0.04
R^2	0.46	0.44	0.37		0.30
Panel D. Age between 14 and 17					
Exposed districts x Post MFI ban	-0.061***	-0.032***	-0.030***	-0.025***	0.017***
	(0.007)	(0.003)	(0.005)	(0.008)	(0.004)
Pre-ban Control Mean	0.19	0.09	0.10	0.04	0.12
R^2	0.66	0.39	0.53	0.47	0.38
District FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	1,809	1,809	1,809	1,809	1,809

Notes: We restrict the sample to districts with at least one MFI in 2008. We aggregated the NSSO data at the district level. For children under 14, we lack observations on vocational training, precluding estimates of its impacts. In panel A, prior to aggregation, we retained observations of children aged 4 to 17. For panels B to D, we similarly prepared the data before aggregation, focusing on specific age groups: 4 to 10, 11 to 13, and 14 to 17 years. "Any" = The portion of children who participate in self-employment activities or work for wage/salary. "Self-Employment" = The portion of children who work in the household enterprise (self-employment). "Wage/Salary" = The portion of children who work at non-household enterprises. "Vocational Training" = The portion of children who receive any form of vocational training, including formal vocational training, non-formal vocational training, self-learning, learning on the job, and others. "Domestic Duties" = The portion of children who engage in domestic duties only. All regressions include district and year fixed effects, with standard errors clustered at the state level. District weights from the synthetic difference-in-difference method are applied. Significance levels are indicated as follows: + $p < 0.15$, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A3: Enrollment & Employment (IHDS, AP vs. Non-AP Districts): Individual Level

	(1)	(2)	(3)	(4)	(5)
Panel A. Overall Enrollment & Employment					
	School	Work			
	Enrollment	Any	Farm/Animal	Business	Wage/Salary
Exposed districts × Post MFI ban	-0.109***	-0.082***	-0.095***	-0.000	0.016*
	(0.023)	(0.026)	(0.026)	(0.003)	(0.008)
Pre-ban Control Mean	0.50	0.02	0.02	0.00	0.00
R^2	0.58	0.60	0.59	0.50	0.52
Panel B. Working Hours					
		Ln(Work Hours)	Ln(Farm Hours)	Ln(Business Hours)	Ln(Wage/Salary Hours)
Exposed districts × Post MFI ban		-0.290***	-0.374***	-0.000	0.106*
		(0.105)	(0.094)	(0.015)	(0.062)
Pre-ban Control Mean		0.04	0.03	0.00	0.00
R^2		0.57	0.57	0.50	0.52
Individual FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	20,826	20,826	20,826	20,826	20,826

Notes: We restrict our IHDS sample to kids who are less than or equal to 11 years old in the 2005 survey. The sample includes districts with at least one MFI in 2008. "School Enrollment" = A dummy variable equal to one if a child is enrolled at a school (e.g., primary school, middle school, secondary school, and higher secondary school) currently and zero otherwise. "Any" = A dummy variable equal to one if a child does farm/animal care work, non-farm business work, or wage/salary work and zero otherwise. "Farm/Animal" = A dummy variable equal to one if a child does farm or animal care work and zero otherwise. "Business" = A dummy variable equal to one if a child does non-farm business work and zero otherwise. "Wage/Salary" = A dummy variable equal to one if a child does wage or salary work and zero otherwise. "Work Hours" include hours of working in farming, animal caring, business, and wage/salary work. "Farm Hours" include hours spent on farm and/or animal care work. "Business Hours" include hours spent on non-farm work. "Wage/Salary Hours" include hours spent on wage or salary work. We take the inverse hyperbolic transformation of the hour-related variables to retain the zeros. All regressions include individual and year fixed effects. We also control for a kid's sex, age, rural or urban, if a household owned or cultivated land, if a household owned livestock, a household's total assets, if the father/mother is literate, if the father/mother has attended school, father's/mother's completed years of education, if father/mother is working, if father/mother has any major morbidity such as heart disease, cancer, and diabetes. Standard errors are clustered at the district level. Significance levels are indicated as follows: + p< 0.15, * p < 0.1, ** p < 0.05, *** p < 0.01.

TABLE A4: Employment (IHDS, AP vs. Non-AP Districts): Households with *Multiple* School-Aged Children

	(1) School	(2)	(3)	(4)	(5)
		Work			
	Enrollment	Any	Farm/Animal	Business	Wage/Salary
Exposed districts × Post MFI ban -0.002	-0.159***	-0.145***	-0.023**	-0.021	
	(0.034)	(0.030)	(0.030)	(0.009)	(0.021)
Exposed districts × Post MFI ban	-0.092*	0.056**	0.043**	0.013*	0.013
× Younger Sibling	(0.051)	(0.025)	(0.017)	(0.006)	(0.022)
Individual FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Pre-ban Control Mean of Elder Sibling	0.72	0.04	0.04	0.00	0.00
R^2	0.62	0.61	0.60	0.52	0.54
N	17,468	17,468	17,468	17,468	17,468

Notes: We restrict our IHDS sample to kids who are less than or equal to 11 years old and have siblings aged less than or equal to 11 years old in the 2005 survey. The sample includes districts with at least one MFI in 2008. "School Enrollment" = A dummy variable equal to one if a child is enrolled at a school (e.g., primary school, middle school, secondary school, and higher secondary school) currently and zero otherwise. "Any" = A dummy variable equal to one if a child does farm/animal care work, non-farm business work, or wage/salary work and zero otherwise. "Farm/Animal" = A dummy variable equal to one if a child does farm or animal care work and zero otherwise. "Business" = A dummy variable equal to one if a child does non-farm business work and zero otherwise. "Wage/Salary" = A dummy variable equal to one if a child does wage or salary work and zero otherwise. "Younger Sibling" = A dummy variable equal to one if the child has older siblings who aged less than or equal to 11 years old in the 2005 survey and zero if the child does not have an elder sibling who aged less than or equal to 11 years old in the 2005 survey. All regressions include individual and year fixed effects. We control for various factors, including the child's sex, age, rural or urban residence, household land and livestock ownership, total household assets, and the literacy and educational background of the parents, including their years of education, employment status, and any major morbidity like heart disease, cancer, or diabetes. Standard errors are clustered at the district level. Significance levels are indicated as follows: + $p < 0.15$, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A5: Employment (IHDS, AP vs. Non-AP Districts): Households with *Multiple* School-Aged Children (By Education)

	(1) School	(2)	(3)	(4)	(5)
		Work			
	Enrollment	Any	Farm/Animal	Business	Wage/Salary
Panel A. All kids' age $<$ 4 in 2005-06 (Primary School)					
Exposed districts × Post MFI ban	-0.067	-0.080**	-0.099***	0.013	
	(0.064)	(0.035)	(0.031)	(0.020)	
Exposed districts × Post MFI ban	0.126**	0.014	0.031^{+}	-0.012	
× Younger Sibling	((0.061)	(0.027)	(0.020)	(0.019)	
Pre-ban Control Mean	0.07	0.01	0.01	0.00	
R^2	0.95	0.60	0.60	0.52	
N	1,280	1,280	1,280	1,280	
Panel B. All kids' age $\in [4,7]$ in 2005-06 (Middle School)					
Exposed districts × Post MFI ban	0.114**	-0.263***	-0.241***	-0.010	-0.022
	(0.052)	(0.060)	(0.049)	(0.020)	(0.036)
Exposed districts × Post MFI ban	-0.337***	0.118**	0.119**	-0.000	-0.020
× Younger Sibling	(0.078)	(0.051)	(0.049)	(0.013)	(0.033)
Pre-ban Control Mean	0.82	0.02	0.01	0.01	0.00
R^2	0.66	0.62	0.62	0.54	0.55
N	1,320	1,320	1,320	1,320	1,320
Panel C. All kids' age $>$ 7 in 2005-06 (Secondary and Higher Secondary School)					
Exposed districts × Post MFI ban	0.117*	-0.277***	-0.141**	-0.065***	-0.034
	(0.062)	(0.082)	(0.066)	(0.022)	(0.082)
Exposed districts × Post MFI ban	-0.172**	0.170*	0.109^{+}	0.015	-0.045
× Younger Sibling	(0.073)	(0.095)	(0.075)	(0.028)	(0.083)
Pre-ban Control Mean	0.93	0.06	0.05	0.00	0.01
R^2	0.70	0.69	0.65	0.58	0.59
N	1,066	1,066	1,066	1,066	1,066
Panel D. One kid's age $<$ 4 and One kid's age $>$ 7 in 2005-06					
Exposed districts × Post MFI ban	0.165**	-0.399***	-0.286***	-0.029**	-0.206***
	(0.077)	(0.072)	(0.065)	(0.014)	(0.042)
Exposed districts × Post MFI ban	-0.124	0.171***	0.090**	0.020*	0.170***
× Younger Sibling	(0.110)	(0.040)	(0.035)	(0.011)	(0.033)
Pre-ban Control Mean	0.87	0.05	0.04	0.01	0.00
R^2	0.77	0.66	0.64	0.57	0.60
N	1,512	1,514	1,514	1,514	1,514
Individual FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Notes: We restrict our IHDS sample to children who are 11 years old or younger and have siblings within the same age range as of the 2005 survey. The sample include districts with at least one MFI in 2008. In panel A, the sample is confined to children from households with only children under 4 years old in 2005. For panel B, the sample is limited to children in households with only children aged 4 to 7 years in 2005. In panel C, we focus on children in households with only children older than 7 years in 2005. Panel D's sample comprises children from households with at least one child under 4 years old and at least one child over 7 years old in 2005. "School Enrollment" = A dummy variable equal to one if a child is enrolled at a school (e.g., primary school, middle school, secondary school, and higher secondary school) currently and zero otherwise. "Any" = A dummy variable equal to one if a child does farm/animal care work, non-farm business work, or wage/salary work and zero otherwise. "Farm/Animal" = A dummy variable equal to one if a child does farm or animal care work and zero otherwise. "Business" = A dummy variable equal to one if a child does non-farm business work and zero otherwise. "Wage/Salary" = A dummy variable equal to one if a child does wage or salary work and zero otherwise. "Younger Sibling" = A dummy variable equal to one if the child has older siblings who aged less than or equal to 11 years old in the 2005 survey and zero if the child does not have an elder sibling who aged less than or equal to 11 years old in the 2005 survey. All regressions include individual and year fixed effects. We control for various factors, including the child's sex, age, rural or urban residence, household land and livestock ownership, total household assets, and the literacy and educational background of the parents, including their years of education, employment status, and any major morbidity like heart disease, cancer, or diabetes. Standard errors are clustered at the district level. Significance levels are indicated as follows: + p$<$ 0.15, * p $<$ 0.1, ** p $<$ 0.05, *** p $<$ 0.01.

B Additional Tables

TABLE C.1: District Statistics (2011 Census of India)

Variable	(1) Mean/SD	(2) Mean/SD	(3) (1)-(2)
Panel A. Districts with MFIs in Other States vs. Districts with MFIs in Andhra Pradesh			
	Other States (189)	Andhra Pradesh (19)	t-test Difference
Ln(Population)	14.447	15.061	-0.614***
	(0.677)	(0.243)	(0.157)
Ln(Households)	12.854	13.657	-0.804***
	(0.673)	(0.255)	(0.156)
Sex ratio	1.048	1.004	0.044***
	(0.044)	(0.018)	(0.010)
Literacy rate	0.595	0.575	0.020
	(0.099)	(0.046)	(0.023)
Literacy rate (Female)	0.518	0.499	0.019
	(0.113)	(0.058)	(0.026)
Working Pop./Total Pop.	0.409	0.479	-0.070***
	(0.067)	(0.030)	(0.015)
Panel B. Zero-Exposure Districts with MFIs in Other States vs. Districts with MFIs in Andhra Pradesh			
	Other States (84)	Andhra Pradesh (19)	t-test Difference
Ln(Population)	14.621	15.061	-0.440***
	(0.664)	(0.243)	(0.155)
Ln(Households)	13.033	13.657	-0.624***
	(0.651)	(0.255)	(0.152)
Sex ratio	1.044	1.004	0.040***
	(0.044)	(0.018)	(0.010)
Literacy rate	0.606	0.575	0.031
	(0.103)	(0.046)	(0.024)
Literacy rate (Female)	0.531	0.499	0.033
	(0.118)	(0.058)	(0.028)
Working Pop./Total Pop.	0.400	0.479	-0.079***
	(0.067)	(0.030)	(0.016)

Notes: Data is from the 2011 Census of India. "Ln(Population)" = Log of total population. "Ln(Households)" = Log of the total number of households in the district. "Working Pop./Total Pop." = Working population as a share of the total population. In Panel A, we compare districts with MFIs located in Andhra Pradesh to districts with MFIs located in other states. In Panel B, we compare districts with MFIs located in Andhra Pradesh to districts with MFIs in other states that were not affected by the Andhra Pradesh ban based on the exposure index developed by Breza and Kinnan (2021). We conduct a two-sample t-test with equal variances in column 3. Significance levels are indicated as follows: + $p< 0.15$, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE C.2: District-Level Enrollment by Age and Education Level (NSSO, AP vs. Zero-Exposure Districts)

	(1)	(2)	(3)	(4)	(5)
Education Level	Overall	Primary	Middle	Secondary	Higher Secondary
Age range	(4∼17)	(4∼10)	(11∼13)	(14∼17)	(14∼17)
Exposed districts × Post MFI ban	-0.003	-0.003	-.039**	-0.046**	0.022^{+}
	(0.011)	(0.008)	(0.014)	(0.019)	(0.015)
Pre-ban control mean	0.59	0.44	0.40	0.33	0.12
R^2	0.82	0.88	0.58	0.59	0.68
District FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
N	1,197	1,197	1,197	1,197	1,197

Notes: The sample includes zero-exposure districts and districts in Andhra Pradesh. We collapsed the NSSO data at the district level. Before collapsing the data, we kept observations belonging to a certain age group (e.g., age between 4 and 17, age between 4 and 10, age between 11 and 13, and age between 14 and 17). All regressions include district and year fixed effects. Standard errors are clustered at the state level. District weights from the synthetic difference-in-difference method are applied. Significance levels are indicated as follows: + p< 0.15, * p < 0.1, ** p < 0.05, *** p < 0.01.

TABLE C.3: Enrollment & Highest Completed Education (IHDS, AP vs. Zero-Exposure Districts)

	(1)	(2)	(3)	(4)	(5)
	Enrolled Now	Primary	Middle	Secondary	Higher Sec.
Exposed districts × Post MFI ban	-0.069**	-0.085**	-0.021	0.077***	0.033*
	(0.028)	(0.042)	(0.027)	(0.017)	(0.017)
Individual FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Pre-ban Control Mean	0.54	0.39	0.01	0.00	0.00
R^2	0.56	0.27	0.60	0.54	0.52
N	9,596	9,596	9,596	9,596	9,596

Notes: We restrict our sample to kids who are less than or equal to 11 years old in the 2005 survey. The sample includes zero-exposure districts and districts in Andhra Pradesh. "Enrolled Now" = A dummy variable that equals 1 if the child is currently enrolled in school. "Primary" = A dummy variable that is set to 1 if the child's highest completed education level is primary school. "Middle" = A dummy variable that is set to 1 if the child's highest completed education level is middle school. "Secondary" = A dummy variable that is set to 1 if the child's highest completed education level is secondary school. "Higher Sec." = A dummy variable that is equal to 1 if the child's highest completed education level is higher secondary school. All regressions include individual and year fixed effects. We also control for various factors, including whether a household owned or cultivated land, whether a household owned livestock, the household's total assets, whether the father or mother is literate, whether the father or mother has attended school, the father's or mother's completed years of education, whether the father or mother is working, and whether the father or mother has any major morbidity, such as heart disease, cancer, or diabetes. Standard errors are clustered at the district level. Significance levels are indicated as follows: + p< 0.15, * p < 0.1, ** p < 0.05, *** p < 0.01.

TABLE C.4: Enrollment & Employment (IHDS, AP vs. Zero-Exposure Districts): Households with *One* School-Aged Kid

	(1) School	(2)	(3)	(4)	(5)
		Work			
	Enrollment	Any	Farm/Animal	Business	Wage/Salary
Exposed districts × Post MFI ban	-0.120*	-0.036	-0.053	-0.003	0.063***
	(0.066)	(0.046)	(0.041)	(0.013)	(0.016)
Individual FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Pre-ban Control Mean	0.55	0.02	0.02	0.00	0.00
R^2	0.56	0.63	0.63	0.52	0.59
N	2,144	2,144	2,144	2,144	2,144

Notes: We restrict our IHDS sample to children who are 11 years old or younger and are the only school-aged children in a household according to the 2005 survey. The sample includes zero-exposure districts and districts in Andhra Pradesh. "School Enrollment" = A dummy variable equal to one if a child is currently enrolled at a school (e.g., primary school, middle school, secondary school, and higher secondary school) and zero otherwise. "Any" = A dummy variable equal to one if a child does farm/animal care work, non-farm business work, or wage/salary work and zero otherwise. "Farm/Animal" = A dummy variable equal to one if a child does farm or animal care work and zero otherwise. "Business" = A dummy variable equal to one if a child does non-farm business work and zero otherwise. "Wage/Salary" = A dummy variable equal to one if a child does wage or salary work and zero otherwise. All regressions include household and year fixed effects. We also control for various factors, including whether a household owned or cultivated land, whether a household owned livestock, the household's total assets, whether the father or mother is literate, whether the father or mother has attended school, the father's or mother's completed years of education, whether the father or mother is working, and whether the father or mother has any major morbidity, such as heart disease, cancer, or diabetes. Standard errors are clustered at the district level. Significance levels are indicated as follows: + p< 0.15, * p < 0.1, ** p < 0.05, *** p < 0.01.

TABLE C.5: Employment (IHDS, AP vs. Zero-Exposure Districts): Boys vs. Girls

	(1) School	(2)	(3)	(4)	(5)
		Work			
	Enrollment	Any	Farm/Animal	Business	Wage/Salary
Exposed districts × Post MFI ban	-0.052^{+}	-0.084**	-0.100***	0.002	0.024*
	(0.035)	(0.034)	(0.034)	(0.004)	(0.013)
Exposed districts × Post MFI ban	-0.070	0.010	0.009	-0.005	0.000
× Female	(0.049)	(0.035)	(0.030)	(0.004)	(0.013)
Individual FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Pre-ban Control Mean	0.54	0.02	0.02	0.00	0.00
R^2	0.58	0.61	0.61	0.51	0.53
N	9,174	9,174	9,174	9,174	9,174

Notes: The sample includes zero-exposure districts and districts in Andhra Pradesh. "Any" = A dummy variable equal to one if a child is doing farm/animal care work, non-farm business work, or wage/salary work. "Work Hours" include hours of working in farming, business, and wage/salary work. "Female" = A dummy variable equal to one if a child is a girl and is zero otherwise. We restrict our sample to kids who are less than or equal to 11 years old in the 2005 survey. All regressions include individual and year fixed effects. We also control for various factors, including whether a household owned or cultivated land, whether a household owned livestock, the household's total assets, whether the father or mother is literate, whether the father or mother has attended school, the father's or mother's completed years of education, whether the father or mother is working, and whether the father or mother has any major morbidity, such as heart disease, cancer, or diabetes. Standard errors are clustered at the district level. Significance levels are indicated as follows: + p< 0.15, * p < 0.1, ** p < 0.05, *** p < 0.01.

TABLE C.6: Employment (IHDS, AP vs. Zero-Exposure Districts): Above vs. Below Poverty Line

	(1) School	(2)	(3)	(4)	(5)
		Work			
	Enrollment	Any	Farm/Animal	Business	Wage/Salary
Exposed districts × Post MFI ban	-0.087***	-0.057*	-0.079**	0.000	0.031***
	(0.028)	(0.034)	(0.033)	(0.003)	(0.009)
Exposed districts × Post MFI ban	0.022	-0.041	-0.048	0.004	0.011
× Below Poverty Line	(0.069)	(0.052)	(0.034)	(0.009)	(0.051)
Individual FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Pre-ban Control Mean	0.55	0.02	0.02	0.00	0.00
R^2	0.58	0.61	0.61	0.51	0.53
N	9,174	9,174	9,174	9,174	9,174

Notes: The sample includes zero-exposure districts and districts in Andhra Pradesh. "Any" = A dummy variable equal to one if a child is doing farm/animal care work, non-farm business work, or wage/salary work. "Work Hours" include hours of working in farming, business, and wage/salary work. We restrict our sample to kids who are less than or equal to 11 years old in the 2005 survey. "Below Poverty Line" = A dummy variable equal to one if the family was below the poverty line using the 2005 definition. All regressions include individual and year fixed effects. We also control for various factors, including whether a household owned or cultivated land, whether a household owned livestock, the household's total assets, whether the father or mother is literate, whether the father or mother has attended school, the father's or mother's completed years of education, whether the father or mother is working, and whether the father or mother has any major morbidity, such as heart disease, cancer, or diabetes. Standard errors are clustered at the district level. Significance levels are indicated as follows: + p< 0.15, * p < 0.1, ** p < 0.05, *** p < 0.01.

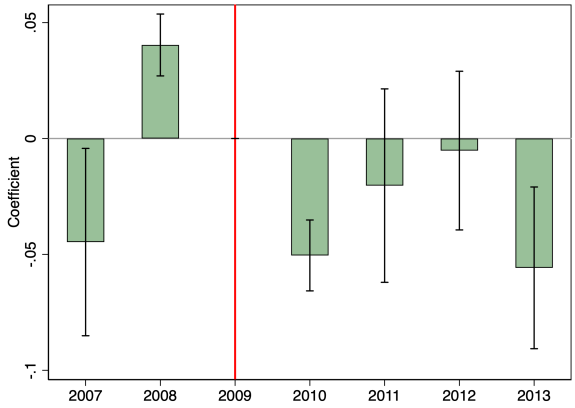
TABLE C.7: Employment (IHDS, AP vs. Zero-Exposure Districts): Low Caste vs. Others

	(1) School	(2)	(3)	(4)	(5)
		Work			
	Enrollment	Any	Farm/Animal	Business	Wage/Salary
Exposed districts × Post MFI ban	-0.086**	-0.055^{+}	-0.071**	0.001	0.028***
	(0.033)	(0.036)	(0.035)	(0.003)	(0.010)
Exposed districts × Post MFI ban	0.009	-0.112**	-0.109**	-0.001	-0.020
× Low Caste	(0.067)	(0.053)	(0.050)	(0.005)	(0.015)
Individual FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Pre-ban Control Mean	0.54	0.03	0.03	0.00	0.00
R^2	0.58	0.61	0.61	0.51	0.53
N	9,174	9,174	9,174	9,174	9,174

Notes: The sample includes zero-exposure districts and districts in Andhra Pradesh. "Any" = A dummy variable equal to one if a child is doing farm/animal care work, non-farm business work, or wage/salary work. "Work Hours" include hours of working in farming, business, and wage/salary work. "Low Caste" = A dummy variable if a child belongs to the Scheduled Castes (SCs) and Scheduled Tribes (STs), which are officially designated groups of people and among the most disadvantaged socioeconomic groups in India. We restrict our sample to kids who are less than or equal to 11 years old in the 2005 survey. All regressions include individual and year fixed effects. We also control for various factors, including whether a household owned or cultivated land, whether a household owned livestock, the household's total assets, whether the father or mother is literate, whether the father or mother has attended school, the father's or mother's completed years of education, whether the father or mother is working, and whether the father or mother has any major morbidity, such as heart disease, cancer, or diabetes. Standard errors are clustered at the district level. Significance levels are indicated as follows: + p< 0.15, * p < 0.1, ** p < 0.05, *** p < 0.01.

C Additional Figures

FIGURE D.1: Event Study Graph (Not Weighted DID): District-Level Enrollment for Children Aged 4 to 16 (ASER, AP vs. Zero-Exposure Districts)



Notes: The sample includes zero-exposure districts and districts in Andhra Pradesh. "Enrolled" = A dummy variable equal to one if a child is enrolled at a primary school, middle school, secondary school, or higher secondary school currently. We collapsed the ASER data at the district level. Before collapsing the data, we kept observations of the age group between 4 and 16. The regression includes district and year fixed effects. Standard errors are clustered at the state level. We do not apply district weights from the synthetic DID. Confidence intervals are at the 95 percent level.